physical surroundings."[1] But such admissions would make no change in the logical aspect of the case; for, however many supplementary causes of this kind we may choose to imagine as possible, the evolutionist is bound to regard them as all alike in this—that they are of a physical or natural kind.

And this leads me to the core of the whole subject. Prof. Gray says:—

"What is probably meant is, that natural selection is a rival hypothesis to design, that it accounts for all adaptations in the organic world on physical principles, and so renders . . . the evidence of design from these adaptations of no other or better value than that from anything else in Nature." He then proceeds to object to this view, and says:—"If means and ends are practicable in inorganic nature at all, it is only by remote and indirect implication; while in organic nature the inference is direct and unavoidable. With what propriety, then, can it be affirmed that organic nature furnishes no other and no better evidence of underlying intelligence than inorganic nature? The evidence is certainly *other*, and to our thinking *better*."

This, I say, is the core of the whole subject. If once it is fully admitted and understood that organic nature is one with all the rest of the universe in the matter of physical causation, so that all the wonderful adaptations which we there encounter are the results of natural causes—survival of the fittest *plus* any number of other natural causes—then it appears to me, as I have said in the essay already alluded to, that all such cases of adaptation must fall into the same logical category, with reference to the question of design, as all or any other series of facts in the physical universe. For the only element of difference arises from the greater intricacy of the physical causation in the cases contemplated, rendering it more difficult to perceive the operation of the causes, at work, and therefore, as Prof. Gray truly asserts, rendering their operation more suggestive of design. But this element of difference does not really affect the question. For, *ex hypothesi*, the law of causation is everywhere and equally uniform, and for this reason the evidence of design in organic nature is certainly *not* other than it is in inorganic nature, nor, in view of the same reason, is it, to our thinking, better.

Florence, February 3 GEORGE J. ROMANES

THE letter of Prof. Asa Gray (NATURE, vol. xxvii. p. 291) contains a sentence which seems to me to contain the essence of the difference between the views of organic life, as held by the supporters of Natural Selection and Natural Theology. He says: "How is this presumption negatived or impaired by the supposition of Darwin's theory, that the ancestors were not always like the offspring, but differed from time to time in small particulars, *yet so as always to be in compatible relations to the environment?*" The italicised portion is just such a statement as "Design" would require, but cannot be held by scientific evolutionists, otherwise why are there so many extinct species? With "Design" there ought to be a perfecting of all species; whereas we know of so many which have been ruthlessly swept aside, owing to their having "differed (or owing to their not having sufficiently differed) from time to time in small particulars, yet" *not* "so as to be in compatible relations to the environment." Change is the evolutionist's view of life—change sometimes caused by the environment, sometimes beneficial, sometimes eventually detrimental: where beneficial, the species increases; where detrimental, other changes or extinction must ensue. Design would never have supplied us with a "Nature red in tooth and claw with ravine," nor would it have built up a system by the expensive and cruel mode of trial and error.

Cove Castle, Loch Long, N.B. J. B. HANNAY

Two Kinds of Stamens with Different Functions in the Same Flower

TO the Melastomaceæ and Commelynaceæ mentioned in NATURE (vol. xxiv. p. 307, vol. xxvi. p. 386, and vol. xxvii. p. 30), may be added the genera *Mollia* (Tiliaceæ), *Lagerstrœmia* (Lythraceæ), and *Heteranthera* (Pontederaceæ), for having differently coloured anthers. In several species of *Mollia*, according to Darwin ("Forms of Flowers," p. 168, footnote), the longer stamens of the five outer cohorts have green pollen, whilst the shorter stamens of the five inner cohorts have yellow pollen; the stigma stands close beneath the uppermost anthers. In a *Lagerstrœmia* in my garden the six outer stamens have green pollen, and are much longer than the numerous inner ones, which have bright yellow pollen; the stigma stands on a level with the outer anthers. I have repeatedly seen bees alighting on, and gathering the pollen of, the inner anthers without noticing the outer ones.

In *Heteranthera reniformis* there is one long stamen (belonging to the outer whorl) having pale bluish pollen, and two short stamens (of the inner whorl) with bright yellow pollen. The stigma stands generally on a level with the anther of the long stamen. When the white flower opens, pistil and long stamen diverge, the pistil bending (almost without exception) to the right, and the stamen to the left; at the withering of the flower, they again approach each other, so that the stigma may be fertilised by the pollen of the long stamen. Visiting insects are attracted yet more to the yellow anthers of the two short stamens by their being placed close to a yellow spot, surrounded by a violet border, at the base of the upper petal.

Thus it may be safely assumed that in all these flowers, as well as in the above-mentioned Melastomaceæ and Commelynaceæ, fertilisation is almost exclusively effected by the pollen of the longer stamens, whilst the shorter stamens serve only to attract pollen-gathering or pollen-eating insects. It is far from surprising that the pollen of these latter stamens, though often produced in large quantity, should tend to degeneration. Darwin long ago came to this conclusion with respect to some Melastomaceæ with differently-coloured anthers, of which he had raised seedlings from pollen both of the longer and shorter stamens ("There is reason to believe that the shorter stamens are tending to abortion."—"Cross- and Self-Fertilisation," p. 298, footnote). The *Lagerstrœmia* in my garden being self-sterile, I fertilised some flowers with green, and others with yellow pollen of a different variety (or species?) growing in other gardens; both produced fruits with apparently good seeds, but only some of those from the green pollen have germinated.

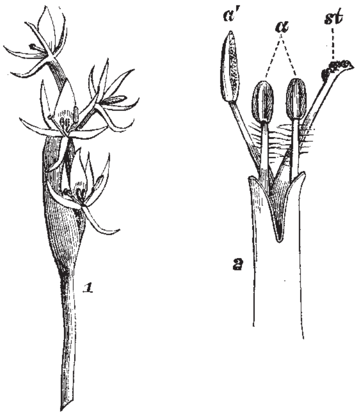

FIG. 1.—Flower-spike of *Heteranthera reniformis* (natural size). FIG. 2.—Upper end of the flower-tube, seen from behind. *a'*, the one anther of the outer whorl, with pale bluish pollen; *a*, the two anthers of the inner whorl, with bright yellow pollen; *st*, stigma.

As in all the flowers above-named, with differently-coloured anthers, the dull colour of those of the longer stamens evidently serves to make them less visible to insects, may not the green colour of the anthers of the long stamens of the mid-styled and short-styled flowers of *Lythrum salicaria* also protect them against the attacks of pollinivorous insects, to which, from protruding far from the corolla, they would be more exposed than those of the shorter stamens?

Even without being differently coloured, the stamens of the same flower may be divided into different sets with different functions. Thus in a species of *Cassia* the visiting humble-bees gather the pollen of the four intermediate stamens (the three upper ones being pollenless), which are short and straight, whilst the three lower ones are very long and curved in such a way that their pollen is deposited on the back of the humble-bees. The pistil is of the same length and curved in the same way as the longer stamens. Another very striking instance has been carefully described by Prof. J. E. Todd of Tabor (Iowa) in

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[1] In my "NATURE Series" essay I expressly stated that natural selection is probably not the only cause of organic evolution, and therefore I think it might have been well if my critic had taken the trouble to refer to this essay before indulging in the general proposition at the close of his letter with reference to exactitude.

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a plant of a very different family, viz. *Solanum rostratum* (*American Naturalist*, April, 1882, p. 281): one stamen and the pistil are very long and strangely curved; four stamens are short and straight, and serve only to furnish pollen to the visiting insects; all the anthers, as I am informed by Prof. Todd, are of the same dull yellow colour.

FRITZ MÜLLER

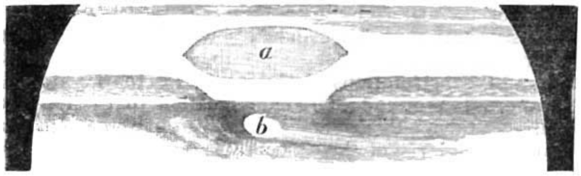
Blumenau, Santa Cattarina, Brazil, December 27, 1882

The Markings on Jupiter

AFTER heavy storms of hail on January 30, the sky cleared and the night was exceptionally fine. I observed Jupiter with my 10-inch reflector about 11h. 30m., and watched the chief markings pass the central meridian of the planet. The well-known equatorial white spot came to transit at 11h. 44m., and it was followed 5 minutes later—at 11h. 49m.—by the great red spot. These objects, therefore, must have been in conjunction on January 30, at 2h. 47m., as the greater velocity of the white spot enables it to gain 13m. 24s. on the red spot daily.

In NATURE, vol. xxv. p. 225, I stated that during the 400d. 0h. 20m. elapsed between 1880, November 19, 9h. 23m. and 1881, December 24, 9h. 43m., the white spot had completed 9 revolutions of Jupiter relatively to the red spot; the number of rotations performed by the former being 976, and by the latter 967. Since 1881, December 24, I have continued to watch the anomalous velocity of these curious markings, and find that between that date and 1883, January 30, the white spot has completed 9 further revolutions of Jupiter. From 1881, December 24, 9h. 43m., to 1883, January 30, 2h. 47m., is 401d. 17h. 4m., during which the white spot has rotated 980 times, while the number for the red spot is 971. In fact my observations since 1880, November 19, show that up to 1883, January 30, the white spot had performed 1956 rotations, as against 1938 by the red spot in the interval of 801d. 17h. 24m.

On January 30, when I last saw these markings, the red spot was remarkable on account of its great faintness. On the other hand, the equatorial white spot was extremely brilliant and conspicuous, and formed one of the most noticeable features on the planet. Observers should now keep a close watch on the red spot, as it seems likely to be on the point of disappearance, though this disappearance need not necessarily be of final character. It fortunately happens that a curious irregularity in the formation of the great southern belt will probably enable the exact position of the spot to be watched for a considerable time. This particular region of the planet, as I drew it on January 30 at midnight, was as follows:—



1883, January 30, 12h. The region south of the equator of Jupiter. *a*, The red spot; *b*, white spot.

The sketch shows that the great south belt is now double, and a very conspicuous object on Jupiter. The south half of this belt is bent abruptly to north, and runs into the other half exactly north of the preceding and following ends of the red spot. There is some explanation to this interesting feature, though it is at present involved in mystery; in any case it may possibly serve as a very accurate indication of the place of the red spot long after that object has become obliterated altogether.

W. F. DENNING

Bristol, January 31

Meteor of November 17

I THINK now that more observations of the remarkable phenomenon of November 17 have been brought forward, that we cannot but candidly acknowledge that the evidence is extremely contradictory and impossible to reconcile, that is as applying to one and the same object. Altogether there is something mysterious about it. It is evident that since it appeared to reach the greatest apparent length of about 30° at York, then from all places further south it ought to have attained a length exceeding this, the more so the further south they are. The ends of the beam appeared very well defined from here, and there was very little room for estimates varying according to the observer's sensitiveness to light. If we take the observations made from Clifton, Cirencester, East Clevedon, Woodbridge, and Windsor, as they nearly all agree in estimating the length as *over* 30°, some considerably over, then these may all relate to the same object. But its appearance from York is flatly contradicted by Mr. Batson's observation from Hungerford, that from Halstead, Essex (which seems to agree with Mr. Batson's), also those from Lincoln's Inn Fields, Greenwich, and Cambridge. All these agree in contradicting the others named above, by assigning a much *smaller* angular length. Mr. Batson describes a sudden foreshortening which the meteoroid underwent when passing the moon, and since I saw it pass below the moon at practically the same time, then (on the supposition that we beheld the same object) the same shortening ought to have been visible to me; but there was not the slightest trace of any such thing. I noticed that it very gradually shortened in length (after allowing for perspective) in its journey towards the west, which is significant, and explainable if we suppose the body to have been encountering resistance to its momentum. It is impossible to reconcile all the observations, and yet most extraordinary that no single observer is known to have witnessed more than one such phenomenon at about that time except Mr. Worthington, who says he saw two *at once.* I have reason to believe that a rather similar thing was seen below the moon at about 5.30 on that night from here. I see that from Ziericksee, in Holland, a similar phenomenon was seen to transit α Pegasi (which would be at about 50° altitude, and on the magnetic meridian from there). If this was the one that I saw, then at the time that it was seen to transit α Pegasi, from Holland, it would appear to me to be just forming in the south-east, where it appeared to be about 10° above the horizon, it which case it would have to be under seventy miles high when over Belgium. But it is almost certain that it attained a height of over 150 miles during the latter part of its course. As yet (figuratively speaking) the spectra of these auroral phenomena have not thrown as much light on these things as that which enters the narrow slip of the spectroscope to print its uncertain record on the retina. I only hope that some one with a clear head and much patience will succeed in unravelling the tangled skeins of evidence which surround the mysterious meteoroid of November 17, 1882.

H. DENNIS TAYLOR

Heworth Green, York, February 11

Aino Ethnology

IN an article on "Aino Ethnology" which appeared in NATURE, vol. xxvi. p. 524, and which I happened to read only a few days ago, Mr. A. H. Keane makes the following statement:—"Until the appearance of Herr Rein's large work on Japan, one of the most universally-accepted of these conclusions was that, whatever be their affinities, the Ainos must certainly be separated from the Mongolic connection. No little surprise was accordingly produced by Rein's attempt to affiliate them to the surrounding members of the yellow race. But it was soon seen that his arguments, apparently inspired by a love of paradox, were sufficiently refuted by the very illustrations of the Aino type introduced into his work."

I submit that one who has read my work upon Japan will decide, with me, that the spirit of the matter quoted is unfair, in so far as it charges me with "attempts" at affiliation, and with being "inspired" otherwise than by a love of truth—this motive being, as stated in my preface, that which induced me to write.

It is repeatedly mentioned in my book that I had never been in the island of Yezo, and those who have carefully read the whole work—including Mr. Keane, if he has done so—cannot reasonably fail to observe that I speak of the Aino tribe as one who had never visited them in their proper home, nor made them a special subject of study in any respect. My remarks upon their probable racial affinities were based upon good, and the then latest, authorities, whose names I was careful to mention. Thus, on p. 444, occurs a passage of which the following is a rendering:—"Doenitz and Hilgendorf have made thorough investigations of their (the Ainos) physical peculiarities, and have published the results thereof in the *Miltheilungen der Deutschen Gesellschaft Ostasiens.* It appeared as an undoubted fact 'that the Ainos are Mongolians, who are separated from the Japanese in a perhaps less degree than the Germans from the Romans.'"

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